Demand Shocks and Firm Investment: Micro-evidence from fiscal retrenchment in Italy*

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Abstract

We study the effect of a persistent demand shock on corporate factor utilization. Our identification strategy leverages a legislative change designed to permanently reduce spending in certain targeted municipalities. This change generated an arguably-exogenous drop in the revenue of procurement firms, which differed depending on each firm's reliance for its revenue on procurement in the targeted municipalities. We find that firms responded to the demand shock by cutting capital rather than

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labor. We propose a theoretical mechanism based on the irreversibility of capital investment.

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1 Introduction

How do firms adjust production factors (i.e., capital and labor) after a persistent demand shock? At a theoretical level, if the shock is negative most theories predict that both factors should be utilized less.[1] If there are adjustment costs, such as firing costs or capital irreversibility, theory suggests that factors should adjust more slowly[2] and, moreover, expectations about shock persistence are likely to play a role.[3] Turning to empirics, most of the macroeconomic evidence on how labor and capital adjust to shocks is aggregate and not causally identified. In fact, not much is known about the causal effect of demand variation on factor utilization due to: (1) the difficulty of identifying plausibly-exogenous demand shocks; and (2) the presence of general equilibrium effects, when the demand shocks happen to be large relative to the economy.

In this paper, we make progress on causal identification by studying an arguably exogenous sector-specific shock, firm-level data, and a small sector of the economy. The demand shock affected some firms more than others for arguably exogenous reasons; we leverage this cross-firm variation to get causal identification. Because we have firm-level data, we can subject our claim of exogeneity to rigorous challenges. Because the sector is small, the variation we rely on is unlikely to give rise to economy-wide confounders or general equilibrium effects. Finally, cross-firm variation in firing costs allows us to assess whether firing costs explain our findings.

The setting is as follows. A law was approved in 2008 that strengthened the enforcement of a pre-existing fiscal rule for Italian municipalities. The law only impacted municipalities with population greater than 5,000. We document that procurement in affected municipalities dropped sharply, relative to those not affected by the law. This drop represented a downward demand *shift* for procurement firms, and this shock was larger for firms with a greater *share* of revenues from affected municipalities. We use this *shift-share* variation to get our estimates.

[1] For example, in real business cycle models demand shocks are introduced as TFP shocks (see, e.g., Benhabib and Wen 2004, p. 515), and "in standard RBC models, a positive technology shock makes both labor and existing capital more productive" Rebelo (2005), p. 11. The same statement applies to network production models such as Acemoglu et al. (2012).

[2] For firing costs, see Sargent (1978) and Bentolilla and Bertola (1990). For capital adjustment costs see Bloom (2009), Shleifer and Vishny (2011).

[3] Bond and Van Reenen p. 4430.

Our headline finding is that firms respond to the demand shock by cutting capital but not labor. To our knowledge, these estimates are the most-credibly identified in the existing literature because our shift-share variation is subjected to rigorous causality tests (pre-trends, placebo tests, etc).

Our secondary finding concerns other margins along which firms adjust to the demand shock. We find that firms do not acquire alternative sources of revenue from non-impacted municipalities, and are not more likely to declare bankruptcy. This feature provides some context for our findings, and it suggests that they may generalize more readily to settings where firms do not make use of these margins.

We conclude by exploring possible mechanisms that could generate our main finding. The labor rigidity we document might be attributed to firing costs. However, the evidence from several distinct empirical approaches uniformly suggests that variation in firing costs does not correlate with response to the shock. Instead, we propose an alternative theoretical mechanism based on the irreversibility of capital investment.

Related literature Nekarda and Ramey (2011) study the impact of variation in government purchases on the US economy. Their level of analysis is more aggregate than ours: an observation is a sector, rather than a firm. Their instrument is year-on-year changes in aggregate government spending interacted with sectoral exposure to government demand. But this instrument may be correlated with unobservables that affect firm performance across sectors and over time.[4] In contrast, our identification comes from "revenue-exposure to municipalities above vs. below the 5k threshold," where exogeneity of the shock to firm-level unobservables is much more plausible. A further point of difference is that Nekarda and Ramey's (2011) time variation comes from an aggregate shock (government spending), and so their estimates incorporate general equilibrium effects across sectors. By contrast, our micro-level analysis is based on a very sector-specific shock and thus isolates the direct effects of a demand shock. Finally, unlike Nekarda and Ramey (2011), we analyze the response to a demand shock we know to be *persistent*. These fundamental differences

[4]For example, if the US government purchases more weapons to fight a foreign war, it need *not* increase its purchases from all sectors in the same proportion (thus violating Nekarda and Ramey's identifying assumption, their eq. 8); furthermore, the US defense sector may concurrently benefit from an increase in "private" sales (perhaps from foreign governments fighting that same war) which is an unobservable shock to demand that is correlated with US government demand (another violation).

may help explain why our findings differ: whereas Nekarda and Ramey (2011) find that capital and labor co-move in response to a demand shock, we find no effect on labor.

Ferraz et al. (2015) study the effects of firm-level demand shocks on employment. Identification is achieved by comparing bidders that narrowly won and lost a Brazilian government procurement auction. While this source of identification is credible, their data lack information on investment, which is the key variable in our paper. In addition, their identification is based on a transitory shock (randomly losing one auction does not imply a reduction in the probability of winning in the future). Similarly, Guiso et al (2005) show that a large cross-section of Italian firms do not pass the burden of temporary productivity shocks through to their employee's wages. Collard-Wexler (2013) studies demand fluctuations in the ready-mix concrete industry. Compared to Collard-Wexler (2013), our analysis is less focused on market structure and more focused on firm-level financial outcomes.

Grembi et al. (2016) study the impact of an earlier (2001) *Patto di Stabilità* on municipal public finance.[5] Chiades and Mengotto (2013) study later versions of the *Patto di Stabilità* using the 5,000 population threshold. Bonfatti and Forni (2016) use the 5,000 population threshold. In these papers the dependent variables are municipal expenditures whereas in our paper they are firm-level outcomes.

Our paper also contributes to the literature on fiscal rules, because the variation in our paper comes from the tighter enforcement of a fiscal rule. This source of variation is policy-relevant because fiscal rules are increasingly common both at the national and at the sub-national level and, often, they are weakly enforced.[6] Our results demonstrate empirically, for the first time to our knowledge, that enforcement is a key determinant of a fiscal rule's effectiveness and that it has an impact on firm investment.

The rest of the paper is organized as follows. Section 2 describes the institutional background and the data. Section 3 shows that the legislative change generated a demand *shift* in some municipalities. Section 4 measures the *share* of firm revenue subject to the

[5]Notably, investment expenditures were exempted from the *Patto* up to 2004.

[6]In 2015, 92 countries had fiscal rules, up from seven in 1990. However, fiscal rules are often weakly enforced. Eyraud et al. (2018, p. 11) writes that "compliance with fiscal rules has been disappointing." Specifically regarding sub-national fiscal rules, Fredriksen (2013, p. 6) reports that "Monitoring and reporting of sub-central fiscal performance is poor and sanctions are not always credible or effective."

demand shift. Section 5 contains the estimates of the effect of the shift-share variation (firm-level demand shock) on capital and labor. Section 6 explores other adjustment margins. Section 7 explores several possible mechanisms for our findings and lands tentatively on capital adjustment costs. Section 8 concludes.

2 Institutional background and data sources

This section discusses the institutional background and the data sources we rely on. Appendix B contains detailed information on the data sources and the variables we use.

2.1 The municipal procurement sector: institutional background

Our sector of interest is municipal public procurement. In Italy, municipal administrations provide roads, schools, and municipal buildings, and they are required to outsource this provision to private contractors via public tenders. The money for these public works is partly raised by the municipality itself, with the balance coming from grants (from the region, the central government, and the EU). Municipal procurement is a very small fraction of GDP (we estimate about 0.3%). Thus a sectoral demand shock is unlikely to reverberate through the economy. This feature makes this sector a good laboratory to study the direct effects of the shock on firms, in isolation of hard-to-measure general equilibrium effects.

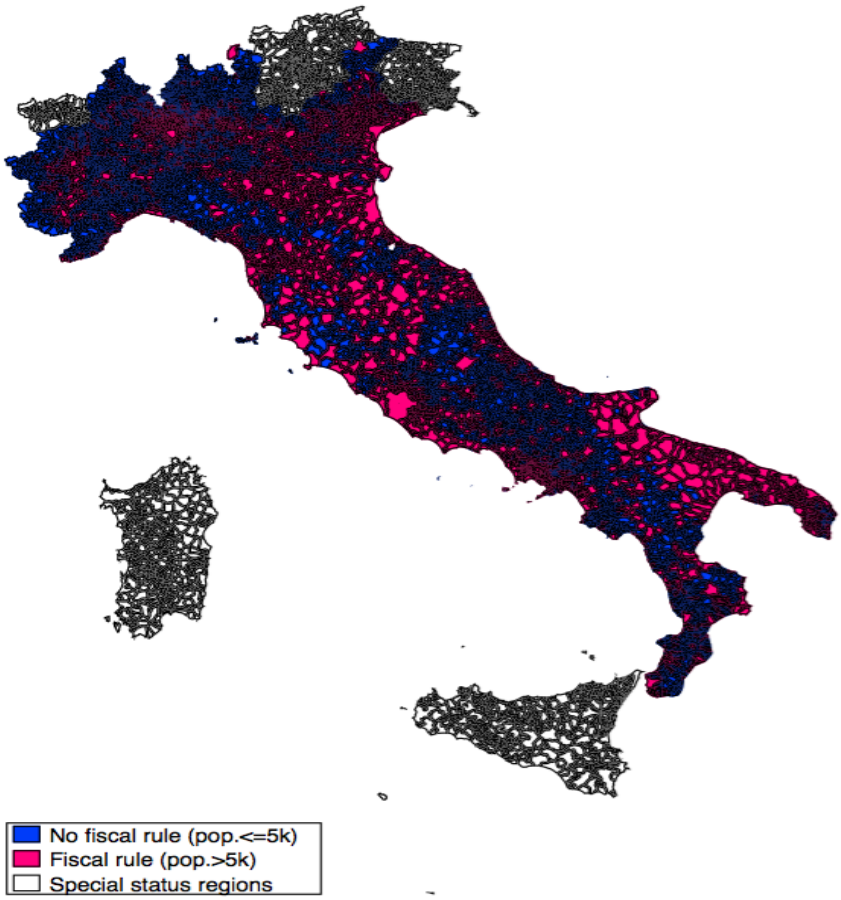
2.2 The legislative change

The *Patto di stabilità dei comuni* is a long-standing fiscal rule designed to check the growth in municipal spending, with a view to controlling municipal debt.[7] Only municipalities with population greater than 5,000 were subject to it during our sample period.

In August 2008, a law was passed unexpectedly that made enforcement of the *Patto* much stricter. For the first time non-compliant municipalities would suffer substantive

[7]During our sample period, the *Patto*: required zero deficit and, moreover, a 20% ceiling on total spending growth (current plus capital, year-on-year); and it did not feature a "golden rule" exempting investments from its purview: see Guerra (2013, p. 954).

Figure 1: Italian municipalities with and without fiscal rule



Notes: Municipalities with population measured in 2001, the most recent census year before the reform. Special status regions, depicted in white, are exempt from the *Patto*; they are *Valle d'Aosta*, *Trentino Alto Adige*, *Friuli-Venezia Giulia*, *Sicilia* and *Sardegna*. Source: Authors' calculation on National Institute of Statistics (ISTAT) data.

cuts in government transfers, and restrictions to borrowing for investment; moreover, mayors and councilors in non-compliant municipalities would receive a 30% salary cut.[8] The new law was aimed at permanently curbing municipal spending. These penalties persisted throughout our sample period and beyond.[9] Following this law, municipalities with population exceeding 5,000 are expected to see a drop in procurement due to the increased enforcement of the *Patto*. This is the "shift" component of the demand shock

[8]Law 133/2008 of August 6, 2008. Articles 77bis *comma* 20, and 61 *comma* 10.

[9]In 2015, for example, the mayor and councilors of Potenza were hit with the penalties: see Brancati (2015).

for firms.

Figure 1 shows the distribution of affected municipalities. In five "special status" regions the *Patto* was optional, and two of them (Sicily and Sardinia) adopted it voluntarily. To guard against endogeneity we drop all five regions from our main estimation sample. We will use the three non-adopting regions to perform placebo tests.

2.3 Municipal procurement data

Municipal budget data do not allow to precisely quantify municipal infrastructure spending. Therefore, we obtained proprietary procurement data from a private company that alerts procurement firms to upcoming tenders. The procurement data includes all the calls for tenders that must be advertised by law, between 2004 and 2011.[10] We use this proprietary data to quantify yearly municipal procurement. Table 1 panel B reports descriptive statistics of the municipal procurement market.[11] In addition, since the data contains information on the tender winners, we match the winners with our firm-level dataset (see next subsection). In this way, we can derive how much procurement business a given firm makes in a given municipality. This quantity will later be used to obtain a firm-level measure of the demand shock.

2.4 Financial information on procurement firms

The AIDA database contains information on all public and privately-owned Italian firms that are required to file a balance sheet.[12] In addition to yearly financial statements,

[10] These are tenders with *valore stimato* above 150,000 euros. See Coviello and Mariniello (2014) for details of advertising requirements.

[11] On average Italian municipalities put out to tender three contracts per year, for a total amount of 976,700 euros. The most-frequently tendered contract type is road construction and maintenance (27%). The average tender attracts 29 bidders. There are 30,788 distinct winners in the database. Bids are expressed as a percentage rebate on a *valore stimato*: this is an estimate of the project's cost which is computed by a municipal engineer based on a government-issued price list. The average winning rebate is 17.02% of *valore stimato*, and the average *valore stimato* is 320,000 euros. On average, 54% of winners are incorporated in the tendering province.

[12] The requirement applies to corporations but not to partnerships. Many procurement-market winners are not found in AIDA, probably because they are partnerships and not corporations, and thus are not

AIDA records the firms' sector (e.g., construction), where the firm is incorporated, and the year of incorporation. AIDA does not report whether a construction firm operates specifically in the public procurement sector. Since we care about firms that operate in public procurement, we restrict attention to the 4,317 AIDA firms that we can match to winners in the procurement-market database described in Section 2.3, and that won at least one infrastructure tender before 2008. See Table 1 panel A for descriptive statistics.[13]

3 The shift: the legislative change

This section describes the "shift" component of our "shift-share" variation in firm-level demand. This shift is caused by the legislative change described in Section 2.2 and results in differential procurement cuts in municipalities with population greater than 5,000, after 2007. We first quantify this differential decrease, and then show that this decrease is unanticipated.

3.1 Measuring the decrease in procurement in large municipalities

To estimate the average impact of the legislative change on municipal infrastructure spending, we estimate the following econometric model:

$$y_{it} = \alpha + \delta Fisc.Rule_i * Post_t + \beta Fisc.Rule_i + \gamma Post_t + \mu X_{it} + \varepsilon_{it}, \tag{1}$$

where y_{it} represents infrastructure spending, $Fisc.Rule_i$ is an indicator variable for municipalities with population above 5,000 (these are the municipalities targeted by the legislative change), and $Post_t$ indicates the years after 2007. Depending on the specification,

required to file a balance sheet. To evaluate how representative our matched data are of the entire sector, we compare matched and unmatched firms based on procurement-market data that are available for both: see Table D.1. Overall, the sample of matched firms is not majorly different from unmatched firms based on observables, even though the differences are often statistically significant due to the large number of observations. Thus restricting to firms that are required to file a balance sheet, as we do, is probably not unrepresentative of the universe of firms.

[13]Before the demand shock takes effect, corporate revenues equal 3.049 million euros on average, only part of which originate from municipal procurement. Wages equal 395,000 euros. Fixed Assets equal 464,000 euros. About 1% of the firms in our matched sample go bankrupt every year.

Table 1: Descriptive statistics (pre-demand shock)

Stats	Mean	St.Dev.	p10	p50	p90	N
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Firm data					
Revenues from Procurement (in 100,000)	4.521	11.45	0	0.341	12.31	10,348
Capital (in 1,000)	449.7	2,861	13.78	114.2	961.5	10,348
Labor (in 1,000)	404.2	1,452	39.39	198.9	801.9	10,348
Fisc.Rule.Exp.	19.01	25.41	0	9.066	53.75	10,348
Municipal exposure	23.29	26.88	1.673	12.80	65.76	10,348
Inc.in Fisc.Rule.Mun.	0.830	0.376	0	1	1	10,348
	Panel B: Municipal procurement data					
Total value of tenders (in 100,000)	9.767	63.37	0	1.912	19.33	24,060
N.Tenders	2.759	8.150	0	1	6	24,060
Avg. value of procurement (in 100,000)	3.192	4.223	0.635	2.080	6.571	15,558
Percent Roads	26.87	35.29	0	7.060	100	15,559
Number of bidders	29.31	25.80	5	22.77	62	7,852
Winning rebate (in %)	17.02	8.134	7.700	15.63	28.46	8,335
Winner from the same province	53.78	34.13	9.028	49.73	100	5,757

Notes: *Revenues from Procurement* is the value of procurement won by a firm in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros); Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities with fiscal rule and the firm's pre-demand shock revenues; *Municipal exposure* represents the ratio between the firm's value won in municipalities (with and without fiscal rule) and the firm's pre-2008 revenues; *Inc. in Fisc.Rule.Mun.* equals one for firms incorporated in municipality with fiscal rule. *Winning rebate* is the municipal-level average of the percentage rebate on the *Avg. value of procurement* (in Italian, *valore stimato*). The latter is an estimate of the project's cost which is computed by a municipal engineer based on a government-issued price list. Source: Statistics pre-demand shock for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011, for all Italian municipalities.

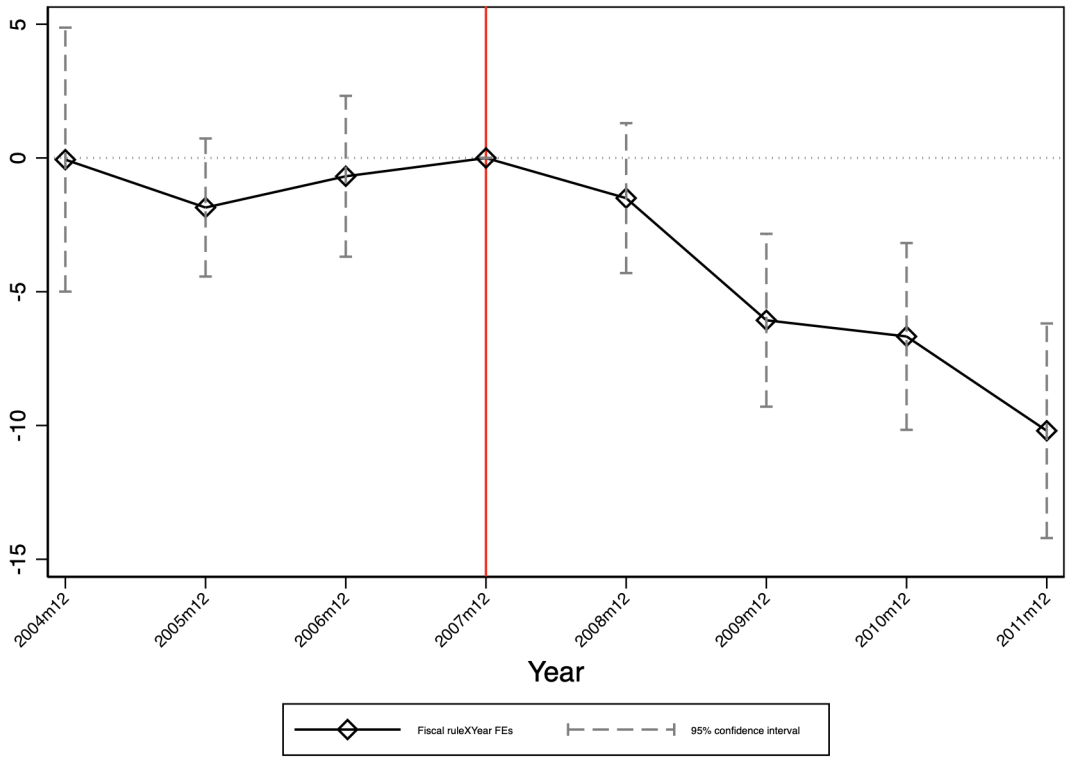
X_{it} includes time-varying municipal population and its squared term, or the population in 2001 interacted with a linear time trend. We include municipal and time fixed effects. Standard errors are clustered at the municipal level. The coefficient δ is the main coefficient of interest: it represents the average impact of the legislative change.

The estimated coefficients of *Fisc.Rule*Post* (Table 2, columns 1 and 2) indicate that infrastructure spending decreased by roughly 23% in municipalities affected by the legislative changes, relative to the unaffected ones. This estimate is stable to including or excluding municipal- and time-fixed effects.

Figure 2 captures the dynamic effects of the legislative change on municipal procurement. This figure is obtained from a variant of equation (1) where the regressor

$Fisc.Rule_i * Post_t$ is replaced by seven $Fisc.Rule_i * Year_t$ interaction terms, with 2007 being the omitted year. The estimated coefficients (see col. 6 of Table 2) are depicted in Figure 2. As expected, after 2007 the value of procurement drops more sharply in municipalities that were impacted by the legislative change.[14]

Figure 2: Dynamic effects of the legislative change on municipal procurement



Notes: The figure reports leads and lags effects of the fiscal shock denoted by the interactions terms between the *Year2004-Year2011, excluding the year 2007* time dummies and the *Fisc.Rule* indicator on municipal procurement. Diamonds represent point estimates while dashed lines 95% confidence intervals. The vertical line indicates the last pre-treatment year. Source: Authors' calculation on procurement data, and municipal budget data from Italian Ministry of Interior for all municipalities between 2004-2011.

[14]That the drop in spending can happen so quickly is consistent with the procurement law (*D.Lgs* 163/06), whereby 3-year municipal procurement plans are revised on yearly basis.

Table 2: Impact of the legislative change on infrastructure spending

Model	Difference-in-Difference Estimates				Parallel Trend	Leads&Lags	Placebo Regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fisc.Rule$_i$*Post$_t$	-5.459***	-5.459***	-5.946***	-5.513***			1.755
	(1.199)	(1.199)	(2.023)	(1.205)			(1.466)
Post$_t$	-0.613***			-0.619***			
	(0.072)			(0.072)			
Fisc.Rule$_i$	21.189***			-2.745			
	(2.266)			(1.928)			
Population$_{i,t}$			2.673				
			(4.209)				
Population$^2_{i,t}$			-0.005***				
			(0.001)				
Pop.$_{i,2001}$*Year$_t$				0.001***			
				(0.000)			
Pop.$^2_{i,2001}$*Year$_t$				-0.001***			
				(0.0001)			
Fisc.Rule$_i$*Year$_t$					0.135		
					(0.774)		
Parallel trend test						0.256	
(p-value sig.)							
Fisc.Rule$_i$*Year2004$_t$						-0.062	
						(2.517)	
Fisc.Rule$_i$*Year2005$_t$						-1.852	
						(1.317)	
Fisc.Rule$_i$*Year2006$_t$						-0.685	
						(1.534)	
Fisc.Rule$_i$*Year2008$_t$						-1.501	
						(1.429)	
Fisc.Rule$_i$*Year2009$_t$						-6.066***	
						(1.648)	
Fisc.Rule$_i$*Year2010$_t$						-6.671***	
						(1.781)	
Fisc.Rule$_i$*Year2011$_t$						-10.197***	
						(2.046)	
Observations	48,120	48,120	48,120	48,112	24,060	48,120	4,200
Municipal FE	NO	YES	YES	NO	YES	YES	YES
Year FE	NO	YES	YES	NO	NO	YES	YES
Mean Y_treat-pre	24.19	24.19	24.19	24.19	24.19	24.19	16.68
Eff.Fisc.Rule on Treated (%)	-22.56	-22.56	-24.58	-22.79	0.559		10.52

Notes: The table reports difference-in-difference estimates of the effects of the legislative change on the annual total value of municipal tenders for infrastructure in all Italian municipalities. In each row, *Fisc.Rule* is an indicator variable for municipalities with population above the 5,000 population threshold and subject to the legislative change, and *Post* is an indicator for the years after 2007. Column 3 controls for time-varying municipal *Population* and its squared term in 1,000 inhabitants; column 4 for *Pop.*$_{2001}$ population in 1,000 inhabitants in 2001 interacted with the time trend (and its squared term). Column 5 reports parametric tests for the parallel trend assumption, by checking the statistical significance of the interaction term pre-2008 *Fisc.Rule*Year*. These estimates are obtained in the pre-legislative change period and are used to test for the presence of linear pre-trends. In column 6, the regression includes leads and lags denoted by the interactions terms between the *Year2004-Year2011, excluding the year 2007* time dummies and the *Fisc.Rule* indicator. In this column *Parallel trend test (p-value sig.)* is the p-value of the joint test for all the leading terms equal zero. Column 7 restricts attention to "special status" regions that chose not to implement the national legislative change (Friuli, Trentino, and Val d'Aosta). Mean Y_treat-pre is the sample mean for treated municipalities pre-2008. *Eff.Fisc.Rule on Treated (%)* is the ratio between the estimated coefficient of *Fisc.Rule*Post* and Mean Y_treat-pre. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2012 in Italy.

3.2 No anticipation of the legislative change, and parallel trends

Visually, Figure 2 shows no evidence of anticipatory effects because the coefficients before 2008 are all small and not statistically different from zero. The parallel trend assumption is supported visually in Figure 2, and it is tested in Table 2 columns 5 and 6. In column 5, the assumption is tested parametrically in a model where the total value of municipal procurement is regressed on a linear time trend, a linear time trend interacted with Fisc.Rule, and municipal fixed effects, in the sample before 2008. The estimated coefficient of the interaction term is small and not statistically significant, suggesting that the parallel trend assumption is not rejected. In column 6, we perform a non-parametric version of this test by checking the statistical significance of the variable *Fisc.Rule* interacted with year dummies (2004-2006), in a model where the total value of municipal procurement is regressed on seven interaction terms $Fisc.Rule_i * Year_{2004} ... Fisc.Rule_i * Year_{2011}$, year and municipal fixed effects, in the 2004-2011 sample. The lack of statistical significance of the pre-2007 individual coefficients, as well as the high p-value of the joint test, both indicate that the parallel trend assumption is not rejected.

To further support the notion that our estimates are not confounded by pre-existing trends that correlate with the impact of the 2008 law, we follow a procedure inspired by Kahn-Lang and Lang (2020) and Goldsmith-Pinkham et al. (2020). We look for X's that predict the impact of the 2008 law with a high R^2, and then toggle $X * time$ in and out of the specification, and check for the stability of the coefficients. An obvious candidate for X is "Population in the municipality pre-treatment," which by definition predicts the impact of the 2008 almost perfectly. Comparing the estimates across specifications that include (col. 4 of Table 2) or exclude (col. 1 and 2 of Table 2) the regressor $X * time$ shows that the estimates are very stable. This is reassuring. Along the same lines, in Table C.3 we produce bias-adjusted estimates that formally account for the possible bias generated by the omission of this variable.[15] Bias-adjusted estimates are comparable to

[15] According to the Oster (2019) procedure, bias-adjusted estimates are computed using the following formula

$$\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}]\frac{R^2_{Max} - \widetilde{R}^2}{\widetilde{R}^2 - \mathring{R}^2}$$

with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being, respectively, the R^2s from the model with and without *Municipal pop. * Year*. The parameter δ is set to 1 under the assumption that firm specific time-varying unobservables are at least as important as the observables to

our main estimates, which is also reassuring.

Finally, one might worry that our estimates may be confounded by mean reversion, i.e.: large municipalities might have had a big increase in infrastructure spending pre-crisis and then, post-crisis, they mean-reverted to their initial level.[16] We believe that Figure 2, showing parallel trends for a full four years before treatment, does not point to mean-reversion. In addition, if "mean reversion for large municipalities" was a factor then we would also expect to see it in placebo regions that were unaffected by the legislative change: but there is no significant effect of "Post" in special status regions (Table 2, col. 7), suggesting that mean reversion is probably not a factor.

3.3 Robustness

First, we check for sorting around the 5,000 municipal threshold. Figure C.1 indicates no evidence of any statistically significant jump in the distribution of the municipal population around the 5,000 population threshold. Second, our results are comparable in magnitude and statistical significance if we consider a less parsimonious model and control for log of municipal population, and for binned categories of population (see Table O.1). Third, we re-do Table 2 using log and inverse hyperbolic sine transformations of the dependent variable (the latter helps guard against zeros in the data). The results are robust; see Table O.2.[17] Finally, in Table C.1 we restrict the sample to tighter windows around the treatment threshold. As the window tightens (1k-10k, 3k-7k, and 4k-6k) around the 5k threshold, the point estimates decrease somewhat but they do not vanish (attaining -17%, statistically significant at the 10% level, in the tightest, least-numerous window).[18] This stability around the threshold supports our interpretation that the procurement drop is due to the legislative change, though we note that it is the *average* treatment effect that provides the "shift" in demand.

estimate treatment effects.

[16] We thank an anonymous referee for this observation.

[17] In Table O.3 we repeat the analysis considering the years after 2008 as "Post" and find evidence that is comparable in size and magnitude to our main evidence.

[18] In Table O.4 we report descriptive statistics of infrastructure spending before the *Patto* around the treatment threshold.

4 The share: fraction of firm revenue exposed to the legislative change

This section describes the "share" component of our "shift-share" variation in firm-level demand. This share, which we call "exposure" to the legislative change, is defined as the percentage of a firm's pre-2008 revenues that originated from procurement in the municipalities targeted by the 2008 legislative change. If revenue origination is somewhat persistent, more-exposed firms are expected to suffer a disproportionate demand reduction after 2008.

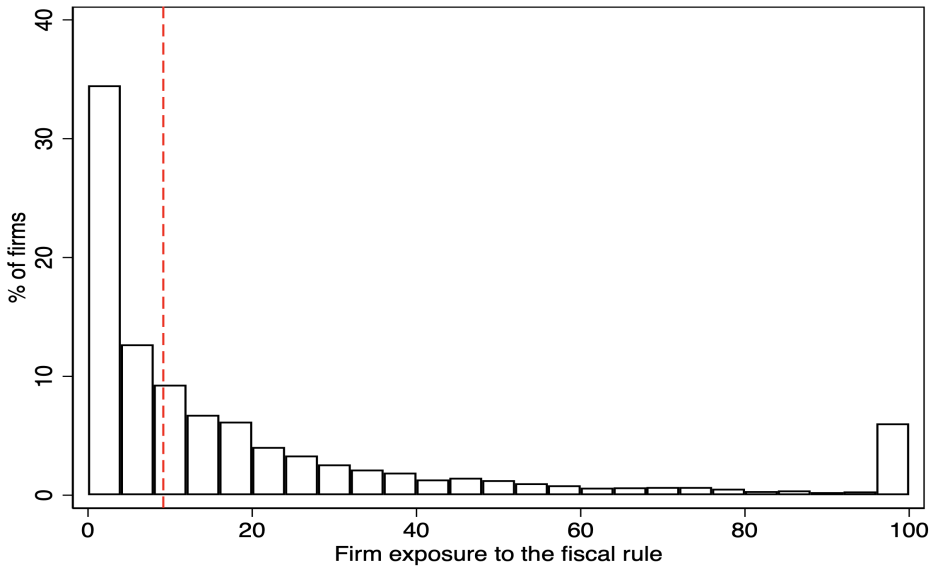
We define a firm's *exposure to the legislative change* $Fisc.Rule.Exp_i$ as the value of procurement won by firm i in municipalities with population greater than 5,000, as a percentage of the firm's total revenues, before 2008.[19] Figure 3 plots the frequency of firms by exposure to the legislative change. The median firm's exposure is just 9%; 23% of firm revenues comes from municipal procurement; 83% of these companies are incorporated in treated municipalities (see descriptive stats in Table 1, panel A). Thus corporations that operate in the municipal procurement sector are revenue-diversified.

5 Main results: shift-share variation

In this section we introduce our shift-share variable, which captures the degree to which a firm is impacted by the legislative change. We then document the absence of differential pre-trends (i.e., we show that the shift-share variable does not correlate with pre-trends in firm-level outcomes). We then analyze in reduced form how the shift-share variable impacted firm revenues, capital, and labor. Next, we argue that the share-shift variable is a valid instrument for a permanent demand shock. Finally, we use the shift-share variable to obtain IV estimates of the effect of a permanent demand shock on capital and labor.

[19] This variable is constructed using the proprietary procurement data described in Section 2.3, so it is only available for the 4,317 companies that won at least one auction before the demand shock.

Figure 3: Heterogeneity in firm exposure to the demand shock



Notes: A firm's exposure to the legislative change is defined as the value of procurement won by a firm in municipalities with population greater than 5,000, as a percentage of the firm's total revenues, before 2008. The sample median is 9% (vertical dashed line) and the standard deviation is 25.41%. Source: Authors' calculation on public works data and AIDA data.

5.1 The shift-share variable

The shift-share variable $Fisc.Rule.Exp_i * Post$ is created by interacting firm i's *exposure to the legislative change* variable defined in Section 4, with a dummy that equals 1 after the legislative change. We estimate the following model:

$$y_{it} = \alpha + \delta Fisc.Rule.Exp_i * Post_t + \beta Fisc.Rule.Exp_i + \gamma Post_t + \mu_c \cdot t + \varepsilon_{it}, \qquad (2)$$

where y_{it} is the variable of interest (revenues, capital, or labor) in levels.[20] To control for municipal-level time trends, we introduce municipality-of-incorporation time trends $\mu_c \cdot t$.[21] In our preferred specification, we also add firm-specific and time fixed effects. Standard errors are clustered at firm level.[22]

[20] In Table O.2 we report estimates of the effects of the demand shock on revenues from procurement, capital and labor expressed in logs, and also applying the inverse hyperbolic sine transformation. We find comparable evidence when we consider these alternative specifications.

[21] Note that this variable is identified as the firms' municipality of incorporation need not be the same as the municipality where companies do municipal business.

[22] In Table O.5 panel A we repeat the analysis with clustering at the municipality-of-incorporation level and find evidence that is comparable in magnitude and statistical significance to our baseline estimates.

5.2 Reduced form estimates: effect of the legislative change on revenues, capital, and labor

As expected, procurement revenue drops more sharply for more-exposed firms after 2007: the estimated coefficient δ from equation (2) is negative in all three specifications, i.e., without firm- and year-fixed effects, with them, and in our preferred specification which further features municipality-of-incorporation time trends (see Table 3, columns 1-3). A one-standard-deviation increase in $Fisc.Rule.Exp_i$, when multiplied by the coefficient in Table 3 column 3, yields $25.41*(-0.101) = -2.57$, corresponding to a drop of 227 thousand euros in annual value of procurement won, or 74% of the average value of municipal infrastructure procurement won. The year-by-year impact of our shift-share variable is reported in column 4 and plotted in Figure 4 panel 1 (top-left): as expected, exposure to municipal procurement is more harmful to procurement revenue after 2008.[23]

Capital decreases more sharply for more-exposed firms after 2007: the estimated coefficient δ is negative across specifications (Table 3 columns 6-9). Based on the estimate from column 8, one standard deviation in $Fisc.Rule.Exp_i$ decreases capital by 15%. This figure is obtained by multiplying $25.41 * (-3.732) = -95$, corresponding to a drop of 95 thousand euros, or 15.4% of average physical assets. Column 10 reports the year-by-year impact of exposure, and the estimates are plotted in Figure 4 panel 2 (top-right): greater exposure leads to greater de-capitalization after 2008.

Labor follows a different pattern from revenues and capital: it does *not* decrease more sharply for more-exposed firms after 2007. Across all specifications, the estimated coefficient δ is not significantly different from zero (Table 3 columns 11-14). Column 15 reports the year-by-year impact of exposure, and the estimates are plotted in Figure 4 panel 3 (bottom-left).

In panel B we repeat the analysis controlling for regional demand shocks by adding a time-varying control for the fraction of cities exposed to *Patto* in the region of incorporation of the company (and its interaction term with the variable $Post_t$) and find evidence that is comparable in magnitude and statistical significance to our baseline estimates.

[23] We replicate our analysis using a different definition of firm-level exposure, one where the denominator is "total revenues from procurement" rather than "total revenues." The point estimates are very similar, but statistical significance is lost because the sample shrinks considerably (total revenues from procurement are zero for many firms in at least one year before the fiscal rule). Results available on request.

5.3 Testing the identifying assumptions: no anticipation, and parallel trends

To visually gauge anticipation effects, Figure 4 plots the estimated coefficients and confidence intervals of the dynamic effects of the *Patto*. These effects are obtained by replacing the regressor $Fisc.Rule.Exp_i * Post_t$ in equation (2) with seven $Fisc.Rule.Exp_i * Year_t$ interaction terms, with 2007 being the omitted year. Visual inspection of the confidence intervals depicted in Figure 4 should alleviate any concern of anticipation effects. A joint test of the coefficients for each of the outcomes (revenues from procurement, capital, and labor) formally confirms this (see Table 3).

In Table 3 column 4 we check whether more- and less-exposed firms share the same trend in revenues from procurement, capital, and labor.[24] We test this parametrically by regressing each outcome before 2008 on a linear time trend interacted with the variable $Fisc.Rule.Exp_i$. The hypothesis of no-pretrends is not rejected for any of the variables (see Table 3, col. 4, 9, and 14). We also look for pre-trends non-parametrically by checking the statistical significance of the variable $Fisc.Rule.Exp_i$ interacted with the year dummies (2004-2006). The lack of statistical significance of all but two of the individual coefficients, as well as the high p-value of the joint test (see Table 3, col. 5, 10, and 15), both indicate that the parallel trend assumption is not rejected.

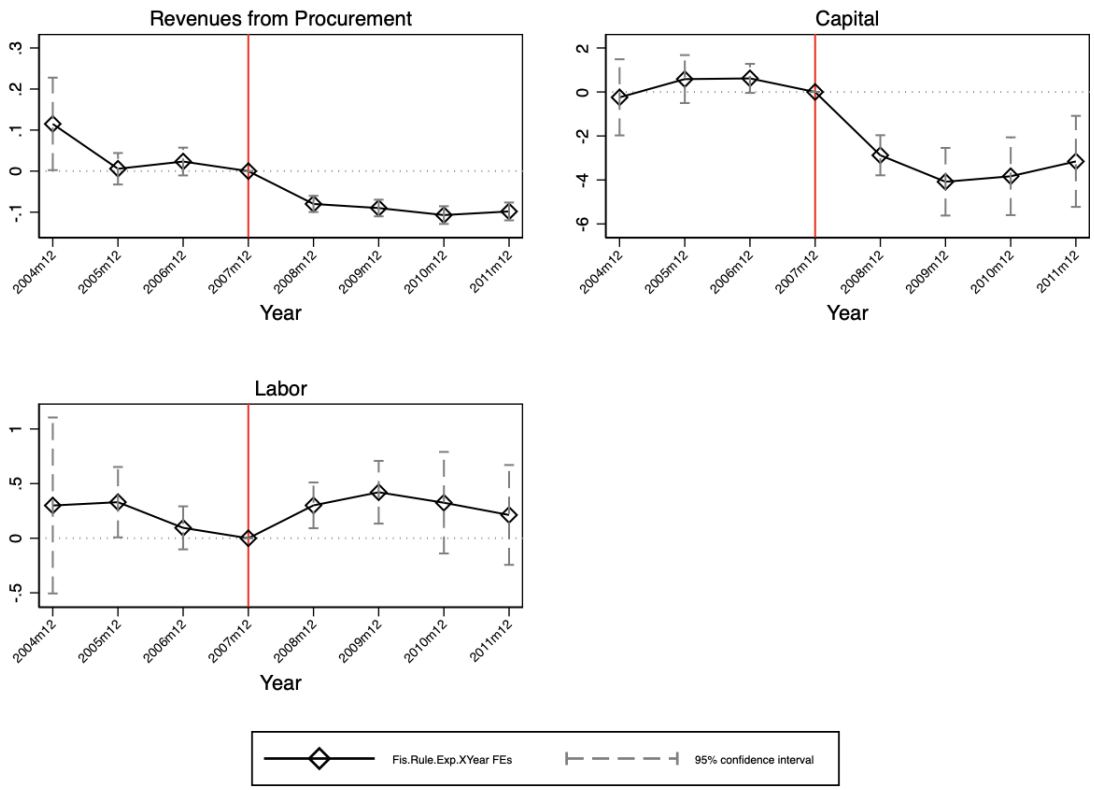
As in Section 3.2, here too we look for regressors that predict treatment – in this case, exposure to the fiscal rule. The predictors with the highest R^2 are municipal fixed effects: regressing *Fisc.Rule.Exp.* on municipal FE returns an R^2 of 0.26 (Table D.3, col.6). We believe there can be many reasons why firms located in municipality A may be more likely than firms located in municipality B to do business with a treated municipality, and we do not seek to explain these reasons. Rather, we note that our $R^2 = 0.26$ is decent within the context of Bartik instruments.[25] Comparing the estimates across specification that include (col. 3, 8, 13 of Table 3) or exclude (col. 2, 7, 12 of Table 3) the regressor $X * time$ we see that the latter estimates are either very similar when the dependent variables are "Revenues from procurement," or "Labor" and actually *increase* in magnitude when the

[24]The standard test for the common-trend assumption needs to be adapted to our setting because our treatment variable (exposure) is continuous and not binary.

[25]For comparison, Goldsmith-Pinkham et al. (2020, p. 2611) deem an R^2 of 0.46 "quite high," and the set of R^2's $\{0.15, 0.095, 0.21, 0.24, 0.02, 0.29\}$ collectively "fair" (p. 2617, referencing their Table 5).

dependent variable is "Capital." Because we see no attenuation when we fail to control for $X * time$, we believe our estimates, which control for $X * time$, are actually conservative. We conclude by following Oster (2019)'s procedure. Table D.4, row 2-4, displays estimates with no city-year FE (col.1), with city-year FE (col. 2), and bias-adjusted estimates (col.3). Reassuringly, most of our estimates of β_s are stable across specifications. This stability suggests that firm-specific time-varying unobservable factors do not represent a major source of bias for our results.

Figure 4: Effect of firm exposure on revenues from procurement, capital and labor



Notes: *Revenues from Procurement* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp*YearFE* is the estimated coefficient of the interaction term *Fiscal Rule Exposure* and the year dummies (*Year2004*-*Year2011*) in a model where outcomes are regressed on: firm and year dummies; the *Fiscal Rule Exposure* variable; the interaction terms; and municipality-of-incorporation time trends (see Columns 5, 10 and 15 of Table 3). Dashed lines are the associated 95% confidence intervals. A firm's exposure to the legislative change *(Fisc.Rule.Exp.)* is defined as the value of procurement won by a firm in municipalities with population greater than 5,000, as a percentage of the firm's total revenues, before 2008. Vertical lines indicate the last pre-treatment year. Source: Authors' calculation on public works data and AIDA data.

Table 3: Reduced form estimates: effect of exposure on revenues from procurement, capital and labor

De.Var.	Rev.Proc	Rev.Proc	Rev.Proc	Rev.Proc	Rev.Proc	Capital	Capital	Capital	Capital	Capital	Labor	Labor	Labor	Labor	Labor
Model	OLS	FE	FE	Parallel Trend	Leads&Lags	OLS	FE	FE	Parallel Trend	Leads&Lags	OLS	FE	FE	Parallel Trend	Leads&Lags
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Fisc.Rule.Exp*Post	-0.110***	-0.105***	-0.101***			-4.239***	-5.177***	-3.732***			0.161	-0.069	0.237		
	(0.007)	(0.007)	(0.008)			(0.569)	(0.685)	(0.823)			(0.210)	(0.205)	(0.171)		
Fisc.Rule.Exp	0.118***					-6.536***					-5.309***				
	(0.007)					(1.261)					(0.629)				
Post	0.155					395.445***					12.763				
	(0.135)					(34.688)					(14.279)				
Fisc.Rule*Year				-0.001					-0.069					0.020	
				(0.015)					(0.192)					(0.099)	
Parallel trend test					0.197					0.122					0.164
(p-value joint sig.)															
Fisc.Rule.Exp*Year2004					0.115**					-0.244					0.300
					(0.057)					(0.883)					(0.411)
Fisc.Rule.Exp*Year2005					0.006					0.585					0.330**
					(0.020)					(0.557)					(0.164)
Fisc.Rule.Exp*Year2006					0.023					0.618*					0.095
					(0.017)					(0.337)					(0.100)
Fisc.Rule.Exp*Year2008					-0.080***					-2.878***					0.301***
					(0.010)					(0.465)					(0.107)
Fisc.Rule.Exp*Year2009					-0.090***					-4.082***					0.421***
					(0.010)					(0.784)					(0.146)
Fisc.Rule.Exp*Year2010					-0.107***					-3.831***					0.325
					(0.011)					(0.902)					(0.237)
Fisc.Rule.Exp*Year2011					-0.098***					-3.156***					0.213
					(0.011)					(1.056)					(0.233)
Observations	22,947	22,891	22,889	10,060	22,889	22,947	22,891	22,889	10,060	22,889	22,947	22,891	22,889	10,060	22,889
Company FE	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
Year FE	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
CityFE*Trend	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Mean Y	3.461	3.461	3.461	3.461	3.456	616.4	616.4	616.4	616.4	617.4	409.9	409.9	409.9	409.9	410.6
St.Dev.Fisc.Rule	25.41	25.41	25.41	25.41		25.41	25.41	25.41	25.41		25.41	25.41	25.41	25.41	
Eff.Fisc.Rule.Exp (%)	-80.85	-76.75	-74.34	-74.34		-17.48	-21.35	-15.39	-15.39		0.996	-0.427	1.467	1.467	

Notes: The table reports estimates of the effects of exposure to the legislative change on firms revenues from procurement, capital accumulation and labor: *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. *Post* is an indicator for the years after 2007. *Year2004-Year2011* are time dummies interacted with *Fisc.Rule.Exp*. When denoted with *Yes* estimates include Company, Year and municipality-of-incorporation time trends *(CityFE*Trend)*. In Columns 4, 9, 14 *Fisc.Rule.Exp*Year* is the interaction term between *Fisc.Rule.Exp* and a linear trend and it is used to test for parametric trends in the pre-2008 sample. In Columns 5, 10, 15 *Parallel trend test (p-value joint sig.)* is the p-value of the joint test for all the leading terms equal zero, and it is used to test for non-parametric pre-2008 trends. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule and Mean Y. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

5.4 The shift-share variable as an instrument for persistent demand variation

In this section we argue that the shift-share variable $Fisc.Rule.Exp_i * Post_t$ is a valid instrument for persistent demand variation.

Strength of the first stage The first stage is strong (Table 3 Col. 3), which qualifies our instrument as relevant.

Validity of the exclusion restriction and multiple treatment concerns The exclusion restriction is satisfied as a matter of logic: the statutory effect of the 2008 law was merely to curb municipal spending. Therefore, we can say that the legislative change operated exclusively through the procurement spending channel. All other indirect effects are caused by reduced municipal procurement spending.

Now, we argue that there is no multiple treatment problem: the effect of the shift-share instrument is not confounded by unobserved correlates of the legislative change. The argument here is necessarily somewhat ad-hoc because there is no principled way of ruling out all conceivable confounders. However, we believe that the argument is compelling in its totality.

1. *Drop in municipal procurement unaffected by post-2007 municipal financial distress*

   Table C.2 col. 1 shows that the size of the drop in municipal procurement is stable in magnitude and statistical significance, if we control for central government transfers to municipalities and for municipal tax revenues. This procedure is crude, however, because it controls for variables that may be endogenous to the treatment: therefore, in col. 2 we control for the pre-treatment value of transfers and tax revenues interacted with the dummy "Post;" and in col. 3 we further allow transfers/taxes to trend differentially for treatment vs control group in the pre- periods.[26] The coefficients remain stable in both specifications. This gives us confidence that the 2008 law, and not some other economic hardship, is responsible for the differential drop in procurement.

[26] We thank an anonymous referee for suggesting this procedure.

2. *Absence of correlation between instrument and pre-2008 municipal procurement*

   We argued in Section 5.3 that municipal procurement trends pre-2008 did not correlate with whether a municipality would be impacted by the shift component of our instrument.

3. *Absence of correlation between instrument and pre-2008 firm-level outcomes*

   We argued in Section 5.3 that more- and less-exposed firms share the same trend in revenues from procurement, in capital, and in labor. Because the shift-share variable does not correlate with pre-trends, it is plausible that the variation provided by the shift-share instrument is "as good as randomly assigned." This claim is further supported by the fact that the estimated changes in revenue from procurement, capital, and labor, are robust to the inclusion of firm fixed effects and municipality-of-incorporation time trends (Table 3 cols. 1 vs. 2, 6 vs. 7, 11 vs 12).

4. *Placebo tests*

   If our instrument was correlated with unobserved confounders, we would expect these confounders to shift procurement in placebo municipalities that were not impacted by the legislative change.[27] We perform three placebo tests on municipal procurement. The first test is on municipalities located in the special status regions that did not adopt the *Patto*. In this sample, procurement should *not* increase differentially post-2007 if the municipality's population exceeds 5,000. Reassuringly, the estimated effect is small and not statistically different from zero (Table 2, col. 7). The second test restricts our main (i.e., non-special status) sample to the pre-2008 period, creates a grid of placebo population thresholds (from 1,000 to 10,000),[28] and runs a diff-in-diff interacting these placebo thresholds with "Post-T" where T are placebo years. Since no municipalities were impacted pre-2008, we expect no effect. Figure 5 panel A reports the p-values of the interaction terms and shows no statistical significance. The third and final municipal placebo test is displayed in Panel B. We drop from our main sample all the municipalities with population above 5,000. For the remaining municipalities, which are not impacted, we generate 10 population

[27] We thank a referee for suggesting these placebo tests.

[28] Municipalities below 10,000 inhabitants represent approximately 90% of the Italian municipalities: see Figure C.2.

placebo thresholds, and interact them with "Post-2007." Reassuringly, we find no effect. These placebo tests show that the estimated revenue drop is driven precisely by the 5,000 population threshold after 2007, and that the estimated effect is not present in "special status" municipalities.

We also perform a firm-level placebo test to guard against firm-level confounders. In Table D.5, the sample is firms located in special status regions that did not adopt the *Patto*, and we use a "placebo shift-share instrument" that is the fraction of those firms' revenues originating from municipalities in their own region with population greater than 5,000 interacted with the dummy post-2007. Reassuringly, we find no effect of the placebo instrument.

Taken together, the placebo tests are strong evidence that the estimated effect is due to the legislative change, and not to some other municipal-level or firm-level confounder.

Interpretation: source of permanent demand variation We conclude that the variable $Fisc.Rule.Exp_i * Post_t$ is a valid instrument for the revenues from procurement, i.e., for the observed "demand shock" experienced by procurement firms. Knowing that our instrument is a (permanent) change in the law allows us to interpret the IV estimates as the firm's response to *permanent*, as opposed to temporary, demand shock. The permanence of this shock is further supported by inspecting the post-2007 coefficients in Figure 2. All the coefficients are negative, decreasing, and statistically significant, indicating that the legislative change has a persistent and growing impact in the years post-2007.

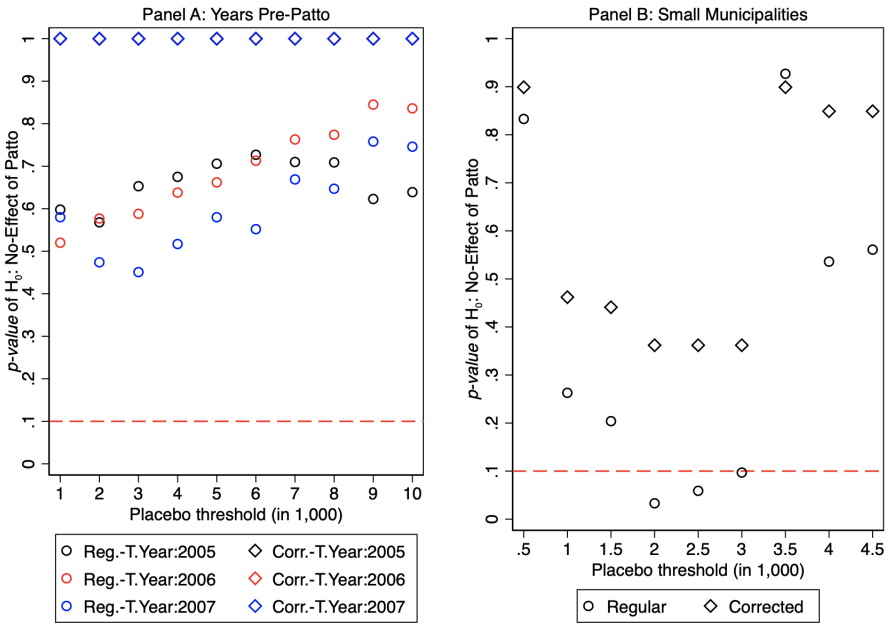
5.5 Instrumental variables estimates

In this section we use the variable $Fisc.Rule.Exp_i * Post_t$ as an instrument for demand. The two-stage model is given by equation (2) together with:

$$x_{it} = \beta_1 + \beta_2 y_{it} + \mu_c \cdot t + \omega_{it}, \tag{3}$$

where x_{it} is either capital or labor, and y_{it} is procurement revenue for firm i in year t. The rest of the variables are the same as in equation (2).

Figure 5: Placebo tests



Notes: The vertical axis represents the *p-value* of the interaction term "Pop. threshold * Post" in a diff-in-diff regression where the dependent variable is procurement spending. Diamonds are Benjamin et al. (2006) *p-values* that correct for multiple hypothesis testing. Panel A retains all the municipalities in our sample, but the years post-2007 are dropped because they were "treated." Within this sample, we create a grid of ten placebo populations thresholds (horizontal axis) and the diff-in-diff specification interacts each threshold with "Post-T" where $T \in \{2005, 2006, 2007\}$ is one of three placebo years (hence every point on the horizontal axis has three p-values: in Panel A there are in fact three diamonds for every threshold but they overlap perfectly at 1). In Panel B we drop the municipalities with more than 5,000 residents because they were "treated," and then generate 10 placebo population thresholds (horizontal axis). The "Post" years in Panel B are those after 2007. None of diamonds is below the 10% threshold. For completeness, we also report standard *p-values* as circles: those, too, do not show a pattern of statistical significance.

Table 4 reports the estimation results. Labor is statistically unaffected by demand variation, both in our OLS and IV estimates. Capital, in contrast, decreases by 36,859-49,521 euros for every 100,000 euros of permanent decrease in demand. The (inverse) output elasticity of capital is approximately 2.[29]

Of note, the estimates for capital are different in the OLS than in the IV. One reason might be that the OLS variation includes temporary variation, which would not cause a firm to shed capital. Our instrument, in contrast, exclusively picks up permanent variation.

[29]The (inverse) output elasticity of capital is $\frac{\delta k}{\delta y} \cdot \frac{y}{k}$. Capital decreases, so $\frac{\delta k}{\delta y} \approx$ 0.37-0.50. The average output and capital in the sample equal 3M and 560k respectively, so $y/k \approx 5$. Therefore, the (inverse) output elasticity of capital equals $5.36 * 0.37 \approx 2$.

Table 4: Instrumental Variable Estimates

Dep.Var.	Capital	Capital	Capital	Capital	Labor	Labor	Labor	Labor
Model	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rev.Proc	-1.096	49.521***	-1.453	36.859***	-1.583	0.660	-2.013	-2.337
	(2.382)	(7.562)	(2.541)	(8.631)	(1.757)	(1.961)	(1.950)	(1.706)
First-Stage F-Stat		559.7		333.2		559.7		333.2
Observations	22,891	22,891	22,889	22,889	22,891	22,891	22,889	22,889
Mean Y	617.5	617.5	617.5	617.5	410.6	410.6	410.6	410.6
Mean Rev.Proc	4.521				4.521			
Company	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CityFE*trend	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The table reports estimates of the effects of revenues from procurement (*Rev.Proc.*) on firms capital accumulation (*Capital*) and labor (*Labor*): *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. All the columns report FE estimates with firm and year fixed effects. When denoted with *Yes* estimates are obtained including municipality-of-incorporation time trends *(CityFE*trend)*. Odd (Even) columns report OLS (IV) estimates (using $Fisc.Rule.Exp_i * Post_t$ as an instrument for *Rev.Proc.*). *Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, and *Post* indicates the years after 2007. *First-Stage F-Stat* is the first stage statistics for the relevance of the instrument. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

6 Other adjustment margins

When hit by a demand shock, firms may adjust on margins other than capital and labor. They may seek new sources of revenue or, *in extremis*, declare bankruptcy. In this section we show that our firms were not successful in replacing lost revenue with revenue from non-impacted municipalities, nor were they more likely to declare bankruptcy if hit by the demand shock.

Seeking new revenue from non-Patto municipalities In this section we are concerned about the possibility that firms which did business in municipalities that were targeted by the 2008 law may, after 2008, start bidding more often in non-targeted municipalities. To explore the economic significance of this effect, we leverage the procurement market data. Table C.4 presents the estimates of equation (1) when the dependent variable

is municipal-level averages of tender outcomes. The outcomes of interest are measures of tender participation and competition: number of bidders (col. 1, 2), winning rebate (col 3, 4), and the fraction of tendered value won by local firms, i.e., firms located in the same province as the tendering municipality (col. 5, 6). We find that tenders held by municipalities targeted by the 2008 legislative change tend to have slightly *more* bidders and slightly *higher* winning rebates after 2007. Furthermore, we detect no effects on the value won by local firms: columns 5-6 show no statistically significant change. Overall, we read the evidence as follows. As expected, targeted municipalities experienced an increase in competition per tender, relative to non-treated ones. The fact that the value won by local firms *did not decrease* in non-targeted municipalities is persuasive evidence that affected firms did not poach on "non-targeted territory," i.e., we do not see firms adjusting on this margin in response to the demand shock.

Bankruptcy Table D.6 reports estimates of the impact of an increase in our shift-share variable on the yearly probability that a firm goes bankrupt. Multiplying the estimate in col. 2 by a standard deviation in exposure to $Fisc.Rule.Exp_i$ yields 0.01*25.41=0.25, that is, a 0.25 percentage points increase in the exit rate of corporations who operate in the municipal procurement sector. After dividing by the 2.26% average exit rate, we get a 11% increase in the exit probability relative to its baseline.[30] This effect is 100 times smaller and not statistically different from zero if we include municipality of incorporation-by-year fixed effects (col.3). Thus, we find no evidence of a sizable "bankruptcy effect" of the fiscal rule.

7 Mechanisms

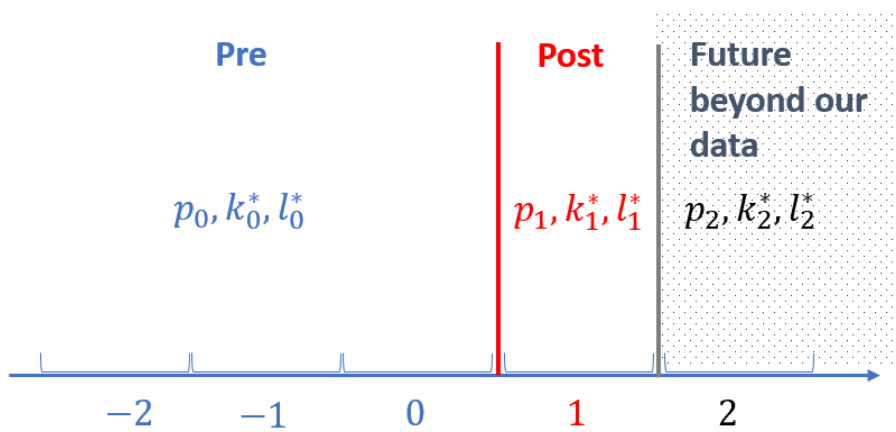
Section 5 has shown that firms cut capital, but not labor, in response to a permanent demand shock. In this section we ask: what kind of theory would rationalize the evidence? We find that a theory without adjustment costs cannot rationalize the evidence. A theory with firing costs can rationalize the evidence, but we will produce direct empirical evidence on firing costs that seems to reject this mechanism in our setting. A theory where capital

[30] We cannot provide results on the effect of $Fisc.Rule.Exp_i$ on the entry rate in Table D.6 because the definition of exposure to treatment limits the sample to corporations that existed before the demand shock hit.

is irreversible can also rationalize the evidence when capital and labor are substitutes, but only if the demand shock is persistent. Since our estimates come from a persistent demand shock, this is our preferred theory.

The timing is as depicted in Figure 6. The pre-period corresponds to pre-2008; period 1 is interpreted as 2008-11 (what we call "Post"); and period 2 is interpreted as the future beyond our data window. The parameters p_0, p_1, p_2 are interpreted as demand levels. Denote by k_t^*, l_t^* the firm's optimally-chosen capital and labor levels in period t. The evidence from Section 5 may then be expressed as follows.

Figure 6: Conceptual framework



Definition 1. ***("the evidence")*** *We refer to the constellation $p_1 < p_0$, $l_1^* = l_0^*$ and $k_1^* < k_0^*$, as "the evidence."*

7.1 No adjustment costs

Consider a firm that operates a production technology

$$y = f(k, l),$$

that satisfies the mild assumption that inputs are normal.[31] With no adjustment costs, the firm's problem is a succession of static maximization problems of the form:

$$\max_{k_t, l_t} p_t f(k_t, l_t) - rk_t - wl_t, \tag{4}$$

where r and w are exogenous factor prices. Because inputs are normal, the optimal k_t^* and l_t^* must co-move with p_t, hence, if p_0 and p_1 are such that $k_1^* < k_0^*$ then it must also be $l_1^* < l_0^*$. Therefore we have the following result.

Lemma 1. ***(the evidence cannot be rationalized without adjustment costs)*** *Suppose k_t and l_t solve (4) and the production function f has normal inputs. Then, regardless of p_0 and p_1, it is not possible that $l_1^* = l_0^*$ and $k^* < k_0^*$.*

This lemma implies that a model without adjustment costs cannot rationalize the evidence. We turn to a model with adjustment costs.

7.2 Labor adjustment costs

A labor adjustment cost of the form $C \cdot |l_t - l_{t-1}|$ can, if added to problem (4), rationalize the evidence. However, we find that variation in C (as proxied by the duration of labor trials, or by whether a firm is large enough to be unionized, or by whether labor is outsourced) fails to correlate with $\Delta l^* = l_1^* - l_0^*$ and $\Delta k^* = k^* - k_0^*$.

The first proxy for firing costs is the duration of labor trials in the court of appeal where the firm is incorporated.[32] Interacting this proxy with our shift-share variable suggests that firing costs do not influence the response of capital and labor to the demand shock (Table D.7 Panel A). The second proxy is whether a firm has more than 15 employees – a threshold above which stricter employment protection law (EPL) applies.[33] To insure

[31] Inputs are normal if they co-move along the expansion paths. Expansion paths are the loci of k and l that give the least-cost production of any given y, for given factor prices. Any homotetic production function has normal inputs, for example.

[32] We use the CEPEJ(2020) measure of duration of trials obtained as the ratio between the total backlog of labor disputes and the cases disposed in 2007 (and multiplied by 365), since data on exact duration does not exist for the years prior to 2008. This measure is used as a proxy for firing costs in Gianfreda and Vallanti (2017) for Italy and similar judicial data is used in Fraisse et al. (2015) to estimate the imapct of firing costs on labor flows in France.

[33] See, e.g., Garibaldi and Violante 2005, Schivardi and Torrini 2008, Lenzu and Manaresi 2018.

comparability, we restrict the sample to firms with employment between 10 and 20 workers before 2008, and create an EPL dummy for firms with average employment greater than 15. Interacting this dummy with our shift-share variable again suggests that firing costs do not influence the response of capital and labor to the demand shock (Table D.7 Panel B). Finally, we consider a form of labor that is not subject to firing costs: outsourced services.[34] If firing costs were the cause of the labor rigidity we measure, we should observe outsourced services to drop after the demand shock. However, we do not observe this (Table D.8).

In sum, a theory of firing costs can rationalize the evidence, but the empirical evidence on firing costs seems to reject this theory as an explanation for the fact that $\Delta l^* = 0 > \Delta k^*$. Therefore, we explore a different mechanism based on capital adjustment costs.

7.3 Capital adjustment costs

It is widely accepted that de-mobilizing sector-specific capital in response to a sector-level demand shock is difficult.[35] Here we provide a model where capital is irreversible, and capital and labor are close substitutes. This model can rationalize the evidence through the following intuitive mechanism. Suppose a firm has to make a capital decision in period 1 that cannot be reversed in period 2, and the firm experiences a negative period-1 demand shock that portends an even worse period-2 shock. Then it is optimal for the firm to immediately reduce capital *beyond the level warranted by the period-1 shock*. This allows the firm immediately to reach a capital level suitable to period 2 (recall that capital cannot be adjusted in period 2), and to attenuate the period-1 distortion by replacing capital with labor in period 1 (this is where input substitutability matters). Thus we expect capital to fall sharply in period 1, and labor to pick up the slack in period 1 (theoretically, labor could even increase in period 1). In period 2, both capital and labor will be very low (note that we do not observe this period in our data).

[34] For example, a school builder might hire a subcontractor to plant a garden, and also hire professional services such as lawyers, accountants, etc.

[35] See, e.g., Shleifer and Vishny (2011), and Bloom (2009) who argues that, in modeling firm-level response to shocks, it is more important to account for capital than labor adjustment costs.

Assume the representative firm operates a linear CES technology of the form:

$$f(k,l) = \sqrt{k^{\rho} + l^{\rho}}.$$

We restrict attention to the case $\rho \in (0,1)$, meaning that inputs are substitutes. We set w equal to r to avoid corner solutions in the region where $\rho 1$ is close to 1. We divide the firm's decision problem into a pre- and a post-demand shock phase. The fact that the firm solves these two problems separately embodies the assumption that the post- regime is unanticipated.

The firm's "pre-demand shock" problem We assume that the environment is stationary before the shock, that is, $p_0 = p_{-1} = p_{-2} = ...$ In a stationary environment adjustment costs do no matter because no adjustment is ever needed. Therefore the optimal strategy in a stationary environment solves the following time-invariant problem:

$$\max p_0 \sqrt{k^{\rho} + l^{\rho}} - rk - rl. \tag{5}$$

When solving (5), firms do not anticipate the future period-1 shock.

The firm's "post-demand shock" problem Starting in period 1 the environment is no longer stationary: we have $p_1, p_2 \neq p_0$. The firm's "post-problem" is:

$$\max_{k,l_1,l_2} p_1 \sqrt{k^{\rho} + (l_1)^{\rho}} + p_2 \sqrt{k^{\rho} + (l_2)^{\rho}} - 2rk - r(l_1 + l_2). \tag{6}$$

This maximization problem embodies the assumption that, while labor is acquired on the spot market in every period (no labor rigidities), capital lasts for both periods (it is irreversible once acquired; for this reason, k is not time-subscripted and the price of capital is doubled). This admittedly stark setting approximates a more realistic setting in which capital is durable but depreciates progressively, the firm can choose to augment its capital stock in every period, and the capital stock cannot be sold on the market. Separately, because p_1 and p_2 are known at the beginning of period 1, problem (6) captures an environment where the unanticipated period-1 shock is fully informative about the period-2 shock.

Proposition 1. ***(capital adjustment costs rationalize the evidence)*** *Denote the solutions to the "pre- problem" (5) and the "post-problem" (6) by* (k_0^*, l_0^*) *and* (k^*, l_1^*, l_2^*)*, respectively.*

1. ***(the evidence can be rationalized)*** *Given any two positive numbers a and $b \in [a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}, a)$, there exist shocks $p_0 > p_1$ such that $(k_0^*, l_0^*) = (a, a)$ solves the "pre-problem" and $(k^*, l_1^*) = (b, a)$ are part of the solution to the "post-problem."*

2. ***(the evidence can only be rationalized if the negative demand shock portends even worse future shocks)*** *If $l_1^* = l_0^*$ and $k^* < k_0^*$, it must be $p_0 > p_1 > p_2$.*

Proof. See Appendix A □

Part 1 says that the evidence from Definition 1, that is, $p_1 < p_0$, $l_1^* = l_0^*$ and $k_1^* < k_0^*$, can be rationalized. The additional feature that $k_0^* = l_0^*$ in part 1 is not a restriction, it is an equilibrium result that follows from the assumption that factor prices are equal and the production function is symmetric. The restriction that $b > a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}$ in part 1 is a technical assumption needed to ensure that period-2 labor is nonnegative: it means that the period-2 shock should not be too severe. The intuition behind part 1 is that the firm anticipates the worse period-2 shock by reducing capital sharply in period 1, and compensates with labor to meet the desired production level in period 1. This asymmetric adjustment helps the firm cope with the constraint that capital is irreversible once acquired, whereas labor is purchased on the spot market. The substitutability between capital and labor ($\rho > 0$) allows for this compensation. The inequality $p_0 > p_1 > p_2$ in part 2 captures a negative period-1 demand shock which portends an even worse shock in period 2: Proposition 1 part 2 shows that this shock configuration is implied by the existing evidence.

Going back to our data, the implication of Proposition 1 is that the drop in municipal procurement in 2008 (demand shock) was interpreted by procurement firms as portending worse news for the future that we do not observe in our data, i.e., the years following 2011.

In summary: while the goal of this paper is not to provide or test a theoretical model, this section argues that the evidence in Section 5 is consistent with firms adjusting substitute production factors subject to capital irreversibility, in response to a persistent demand shock.

8 Conclusions

We studied the effect of a persistent demand shock on corporate factor utilization. Our identification strategy leveraged a legislative change designed to permanently reduce spending in certain targeted municipalities. This change generated an arguably-exogenous drop in the revenue of procurement firms, which differed depending on each firm's reliance for its revenue on procurement in the targeted municipalities. Because we were able to pinpoint the source of demand variation, we are able to argue that the variation was persistent (in the sense that the legislative change was not reversed) and that it did not generate general-equilibrium confounding effects (because municipal procurement is a tiny fraction of GDP). Due to this combination of features, our research design can deliver what we believe are the best-identified causal estimates of how individual firms adjust their capital and labor in response to a permanent demand shock.

Our main finding is that firms responded to the demand shock by cutting capital rather than labor. This labor rigidity could be attributed to firing costs; however, using several empirical approaches, we found that variation in firing costs does not correlate with response to the shock. Therefore, we proposed an alternative theoretical mechanism based on the irreversibility of capital investment.

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Appendix

A Analytical appendix

Proof of Proposition 1

Proof. **The pre-problem** The firm's first order conditions in the pre- problem (5) are:

$$k \quad : \quad (k_0)^{\rho-1} \frac{p_0 \rho}{2\sqrt{(k_0)^\rho + (l_0)^\rho}} = r \tag{7}$$

$$l \quad : \quad (l_0)^{\rho-1} \frac{p_0 \rho}{2\sqrt{(k_0)^\rho + (l_0)^\rho}} = r \tag{8}$$

implying a symmetric equilibrium

$$k_0^* = l_0^*.$$

Substitute k for l into (7) and isolate k to get:

$$k_0^* = \left(\frac{p_0 \rho}{2r\sqrt{2}} \right)^{\frac{2}{2-\rho}}. \tag{9}$$

When $\rho \to 1$ we have:

$$k_0^* = l_0^* \to \frac{1}{2} \left(\frac{p_0}{2r} \right)^2. \tag{10}$$

The post-problem The first order conditions in the firm's post- problem (6) read:

$$k \quad : \quad k^{\rho-1} \left(\frac{\rho p_1}{2\sqrt{k^\rho + (l_1)^\rho}} + \frac{\rho p_2}{2\sqrt{k^\rho + (l_2)^\rho}} \right) = 2r \tag{11}$$

$$l_1 \quad : \quad (l_1)^{\rho-1} \frac{\rho p_1}{2\sqrt{k^\rho + (l_1)^\rho}} = r \tag{12}$$

$$l_2 \quad : \quad (l_2)^{\rho-1} \frac{\rho p_2}{2\sqrt{k^\rho + (l_2)^\rho}} = r \tag{13}$$

Proof of part 2. To show that $p_1 > p_2$, combine (11-13) to get:

$$(k^*)^{1-\rho} = \frac{(l_1^*)^{1-\rho} + (l_2^*)^{1-\rho}}{2} \tag{14}$$

Since in our case $k^* < k_0^* = l_0^* = l_1^*$, it must be that $l_2^* < k^* < l_1^*$. Note that the LH sides of (12-13) both equal the same decreasing (because $\rho \in (0,1)$) function of l_t multiplied by p_t. Since the two LHS must be equal and $l_1^* > l_2^*$, it must be $p_1 > p_2$.

Let us now show that $p_0 > p_1$. The LH sides of (8) and (12) must be equal. By assumption $l_0^* = l_1^*$ and $k_0^* > k^*$, therefore (because $\rho \in (0,1)$) it must be $p_0 > p_1$.

Proof of part 1.

Substitute $k_0^* = a$ into (9) to get a unique $\overline{p}_0$. Substituting $l_1^* = a$ and $k^* = b$ into equation (14) yields:

$$l_2^* = \left[2\,(b)^{1-\rho} - (a)^{1-\rho}\right]^{\frac{1}{1-\rho}} \stackrel{\text{def}}{=} c$$

Nonnegativity of l_2^* requires $2\,(b)^{1-\rho} \geq (a)^{1-\rho}$, i.e.:

$$b \geq a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}.$$

Having selected l_2^* as the solution of (14) guarantees that, if (12-13) hold, then (11) also holds. Thus, we have reduced the problem to selecting two numbers $\overline{p}_1, \overline{p}_2$ that solve (12-13), i.e.:

$$\begin{aligned}
(a)^{\rho-1}\,\frac{\rho p_1}{2\sqrt{b^\rho + (a)^\rho}} &= r \\
(c)^{\rho-1}\,\frac{\rho p_2}{2\sqrt{b^\rho + (c)^\rho}} &= r.
\end{aligned}$$

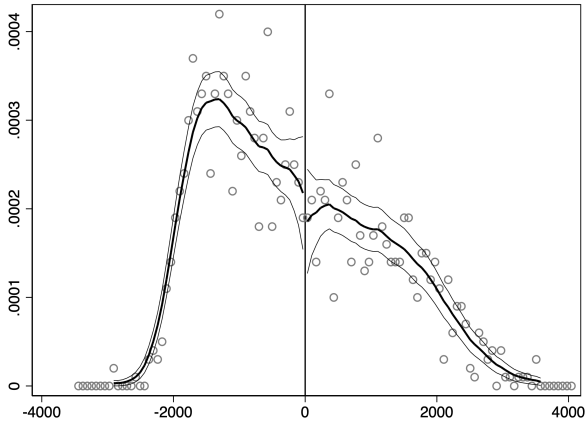
Isolating p_1, p_2 from the above equations yields the desired $\overline{p}_1, \overline{p}_2$. Since $a > b$, by virtue of part 1 it is the case that $\overline{p}_0 > \overline{p}_1$. $\square$

B Variables, Descriptions, and Sources

Variable	Description	Source
Municipalities		
Total value of procurement	Is the annual total value of municipal contests for public works.	Information provider
N.Tenders	Is the number of tenders in a municipality in a year.	Information provider
Avg. value of procurement	Is the average value of the contests tendered in a municipality in a year computed using the engineers' estimates of the value of the contest.	Information provider
Percent Roads	Is the fraction of roads' contests	Information provider
Winning rebate	Is the offer that won the procurement, which represents the percentage discount over the engineer's estimate of the value of the tander. A higher offers represents lower municipal procurement costs.	Information provider
Winner from the same province	Is the value won by firms from the same province of the municipality running the contest over the value of contests by year and municipality.	Authors' calculation on Information provider data
Transfers	Are the annual transfers to the municipality by the central governments (state and region).	Italian Ministry of Interior.
Tax revenues	Are the annual tax revenues of the municipality.	Italian Ministry of Interior.
Total Current (Capital) Spending	Is the annual total current (capital) spendings of the municipality.	Italian Ministry of Interior. Variable: *Totale spese correnti (conto capitale), impegni*
Population	Is the municipal population.	National Institute of Statistics (ISTAT).
Firm balance-sheet		
Bankruptcy/Exit	Probability that a company goes bankrupt in a given year obtained with the year fo last official submission of the balance-sheet.	AIDA. Variable: *Anno ultimo bilancio.*
Rev.Proc	Is the value of procurement contests won in a year (in 100,000 euros).	Authors' calculation on Information provider data
Capital	Total annual physical assets (in 1,000 euros).	AIDA. Variable: *Totale Immobilizzazioni Materiali.*
Labor	Total annual personnel costs (in 1,000 euros).	AIDA. Variable: *Totale salari e stipendi.*
O.Services	Are the firm total costs for outsourced services (in 1,000 euros)	AIDA. Variable: *Servizi*

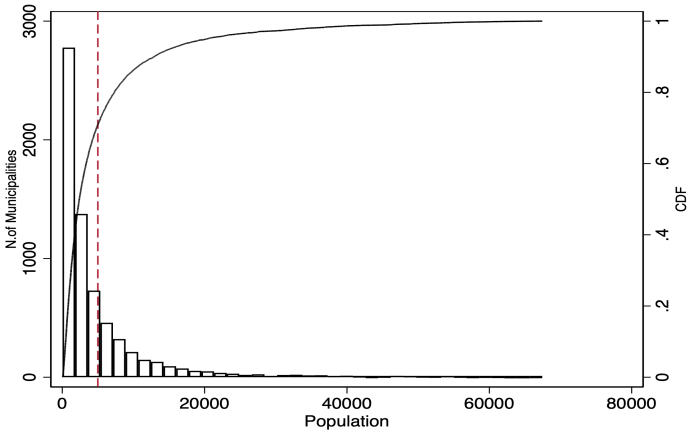
C Additional municipal analysis (the shift)

Figure C.1: No sorting around the fiscal-rule population threshold



Notes. Distribution of the municipal population around the threshold in Italian municipalities with population between 3,000 and 7,000 inhabitants in 2007. Circles represent the difference between the municipal population and the 5,000 threshold (vertical line). Circles are average observed values, the bold solid line is a kernel estimate (see McCrary, 2008), and the two thin lines are 95% confidence intervals. Discontinuity estimate, log difference in height, (and standard errors are -.15 (.26), respectively. Source: Statistics for all the public works tendered between 2004 and 2011 in Italy with population between 3,000 and 7,000 inhabitants in 2007.

Figure C.2: Distribution of Municipalities by Population



Note. All Italian municipalities with less than 68,000 inhabitants in 2007. These municipalities represent the 99% of the approximately 6,800 municipalities.

Table C.1: Impact of the fiscal shock on infrastructure spending for different windows around the 5k threshold

	Main Effect	Parallel Trend	Leads&Lags	Main Effect	Parallel Trend	Leads&Lags	Main Effect	Parallel Trend	Leads&Lags
Municipalities		1k-10k			3k-7k			4k-6k	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fisc.Rule*Post	-1.045***			-0.637			-1.099*		
	(0.308)			(0.416)			(0.582)		
Fisc.Rule*Year		0.244			0.130			-0.051	
		(0.170)			(0.235)			(0.309)	
Fisc.Rule*2004			-0.717			-0.269			0.561
			(0.550)			(0.752)			(0.985)
Fisc.Rule*2005			-0.690			-0.462			0.234
			(0.543)			(0.712)			(1.003)
Fisc.Rule*2006			-0.406			0.033			1.407
			(0.611)			(0.825)			(1.210)
Fisc.Rule*2008			-0.115			-0.013			-0.528
			(0.657)			(0.826)			(1.074)
Fisc.Rule*2009			-1.375**			-1.153			-0.782
			(0.624)			(0.799)			(1.222)
Fisc.Rule*2010			-1.535**			-0.789			-0.273
			(0.666)			(0.882)			(1.222)
Fisc.Rule*2011			-2.969***			-1.292*			-0.613
			(0.567)			(0.738)			(0.878)
Municipalities		3930			1497			701	
Observations	31,440	15,720	31,440	11,976	5,988	11,976	5,608	2,804	5,608
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	YES	YES	NO	YES	YES	NO	YES
Mean $Y\_treat - pre$	7.646			6.978			6.531		
Eff.Fisc.Rule(%)	-13.67			-9.131			-16.83		
Parallel trend (p-value)			0.791			0.777			0.533

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. *Fisc.Rule*Year* is the interaction between *Fisc.Rule* and the linear trend. These estimates are obtained in the pre-2008 sample and are used to test for the presence of linear pre-trends. *Parallel trend (p-value)* is the *p-value* for the joint statistical significance of the leads effect of the demand shock. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy with population between 1,000 and 10,000 (cols. 1 and 2); 3,000 and 7,000 (cols. 3 and 4); 4,000 and 6,000 (cols. 5 and 6).

Table C.2: Robustness: Impact of the fiscal shock on infrastructure spending controlling for transfers and tax revenues

	(1)	(2)	(3)
Fisc.Rule*Post	-5.098***	-6.167**	-5.626**
	(1.108)	(2.779)	(2.869)
Fisc.Rule	4.732*	3.857	3.856
	(2.537)	(2.894)	(2.892)
Post	-0.609***	-0.673***	-1.226***
	(0.079)	(0.185)	(0.156)
Observations	47,806	47,802	47,802
Mean Y_treat-pre	24.15	24.15	24.15
Eff.Fisc.Rule on Treated (%)	-21.11	-25.54	-23.30

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. In Column 1 we include as controls yearly transfers (in 100k) that represents the transfer to the municipality by central governments (state and region), and yearly taxes (in 100k) that represent municipal tax revenues; in Column 2, we include only pre-*Patto* values of the transfers and taxes interacted with *Post*; in Column 3 we also allow them to trend differentially for treatment vs control group in the pre-period. All the regressions control for population in 1,000 inhabitants. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

Table C.3: Bias adjusted municipal level estimates

Model Desc.	Short: No $Pop._{i,2001} \cdot Year_t$ $\mathring{\beta}$ $(St.\mathring{E}rror)$ $[\mathring{R}^2]$ (1)			Long: Yes $Pop._{i,2001} \cdot Year_t$ $\widetilde{\beta}$ $(St.\tilde{E}rror)$ $[\widetilde{R}^2]$ (2)			Bias-Adjusted β^* R^2_{Max}=1.3·$\widetilde{R}^2$ (3)
All	-5.46	(1.2)	[.0282]	-5.51	(1.2)	[.649]	-5.53
1k-10k	-1.05	(.308)	[.0304]	-1.05	(.308)	[.0391]	-1.06
3k-7k	-.637	(.416)	[.011]	-.64	(.417)	[.013]	-.647
4k-6k	-1.1	(.582)	[.004]	-1.1	(.582)	[.005]	-1.1

Notes: In column 1(2), estimates are obtained without (with) including the variable $Pop._{i,2001}$ interacted with the time trend in a model that includes *Fisc.Rule* the indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants), and *Post* the indicator for the years after 2007, and their interaction term. In column three bias-adjusted estimates β^* are computed with the following formula, and implemented in STATA with the .ADO file PSACALC2: $\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}]\frac{R^2_{Max}-\widetilde{R}^2}{\widetilde{R}^2-\mathring{R}^2}$ with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being the R^2_s from the model with and without$Pop._{i,2001} \cdot Year_t$, respectively. δ is set to 1 under the assumption that city specific time-varying unobservables are at least as important as the observables to estimate treatment effects. Rows reports estimates for all, 1k-10k, 3k-7k, and 4k-6k municipalities.

Table C.4: Checking for treatment spillovers in the procurement market

Dep.Var.	N.Bidders		Winning Rebate		Winners from the same province	
Model	OLS	FE	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Fisc.Rule*Post	2.837**	1.618	1.055***	0.903***	0.596	-0.714
	(1.131)	(1.303)	(0.292)	(0.319)	(1.309)	(1.559)
Post	5.659***		2.677***		6.145***	
	(0.785)		(0.226)		(1.013)	
Fisc.Rule	8.116***		1.510***		-16.877***	
	(0.681)		(0.252)		(1.011)	
Observations	13,520	12,312	16,310	15,100	11,277	9,956
Municipal FE	NO	YES	NO	YES	NO	YES
Year FE	NO	YES	NO	YES	NO	YES
Mean Y_treat-pre	32.77	32.77	17.68	17.68	47.02	47.02
Eff.Fisc.Rule on Treated (%)	8.656	4.938	5.966	5.109	1.267	-1.519

Notes: The table reports estimates of the effects of the demand shock on municipal procurement outcomes: *N.Bidders* is the number of competitors submitting an offer; *Winning-Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. Odd (even) columns report OLS (FE) estimates (with municipal and year fixed effects). Mean Y_treat-pre and St.Dev. Y_treat-pre are the sample mean and standard deviation for treated municipalities pre-2008. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

D Additional analysis of firm's responses

Table D.1: Descriptive statistics by matched and unmatched firms (pre-fiscal demand shock)

Sample	Unmatched	Matched	T-test
	(1)	(2)	(3)
Share Proc.Won	25	75	
Proc.Won in *Patto* Mun.	0.78	0.79	0.006
N.Tenders	1.17	1.51	0.344***
Val.Proc.	4.61	5.46	0.842***
Roads	0.27	0.32	0.054***
N.Bidders	34.55	37.64	3.093***
Winning rebate	18.11	17.82	-0.291
Winner same prov.	0.45	0.48	0.037***

Notes: Table reports statistics at contract level by matched and unmatched firms. *Share Proc.Won* is the share of procurement; *Proc.Won in Patto Mun.* indicates contracts won in a Municipality with population above 5k. *N.Tenders* is the number of tenders won; *Val.Proc.* is value of tenders ; *Roads* indicates co*N.Bidders* is the number of competitors submitting an offer; *Winning Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. *Winner same prov.* indicates winners of the contracts from the same province of the municipality.

Table D.2: Descriptive statistics for matched and unmatched firms (pre-fiscal demand shock)

Stats	Mean	St.Dev.	p10	p50	p90	N
	(1)	(2)	(3)	(4)	(5)	(6)
Proc.Won in *Patto* Mun.	0.781	0.413	0	1	1	39,651
N.Tenders	1.254	1.413	0.250	0.750	2.750	39,651
Val.Proc.	4.824	9.861	0.500	2.267	10.24	37,909
Roads	0.281	0.450	0	0	1	39,651
N.Bidders	35.31	28.58	8.270	27.84	71.68	34,758
Winning rebate	18.03	10.39	6.950	16.02	32.72	37,359
Winner same prov.	0.456	0.498	0	0	1	39,081

Notes: Table reports statistics at contract level for the sample of matched and unmatched firms. *Proc.Won in Patto Mun.* indicates contracts won in a Municipality with population above 5k. *N.Tenders* is the number of tenders won; *Val.Proc.* is value of tenders ; *Roads* indicates co*N.Bidders* is the number of competitors submitting an offer; *Winning Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. *Winner same prov.* indicates winners of the contracts from the same province of the municipality.

Table D.3: Correlates of exposure to the demand shock: firm outcomes and geographical characteristics

Charact.	Capital	Labor	Mun. of Inc.>5k	Region FE	Province FE	City FE
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	-0.001	-0.002*	9.058***			
	(0.001)	(0.001)	(1.011)			
R2	0.003	0.009	0.016	0.109	0.144	0.259

Notes: The table reports the estimated coefficients from OLS regressions of *Fisc.Rule.Exp* on: average firms outcomes before 2009: *Capital* are the firm total annual physical assets (in 1,000 euros), *Labor* are the firm total personnel costs (in 1,000 euros); and on firms geographical characteristics: *Mun of Inc. >5k* is an indicator that equals one for firms incorporated in municipalities with population above 5k and exposed to the fiscal rule; *Region FE* are region-of-incorporation fixed effects, *Province FE* are province-of-incorporation fixed effects, and *Municipal FE* are municipality of incorporation fixed effects. Financial variables are deflated using KLEMS deflators. SEs, in parenthesis, are clustered at the firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2008.

Table D.4: Bias-adjusted firm level estimates

Model	Short: No cityFE*trend			Long: Yes cityFE*trend			Bias-Adjusted β^*
Desc.	$\mathring{\beta}$ ($St.\mathring{E}rror$) [$\mathring{R}^2$]			$\widetilde{\beta}$ ($St.\tilde{E}rror$) [$\widetilde{R}^2$]			R^2_{Max}=1.3$\cdot\widetilde{R}^2$
	(1)			(2)			(3)
Rev.Proc	-.11	(.007)	[.047]	-.11	(.008)	[.179]	-.109
Capital	-4.24	(.569)	[.007]	-3.02	(.533)	[.114]	-2.35
Labor	.161	(.21)	[.007]	.301	(.216)	[.11]	.376

Notes: In column 1(2), estimates are obtained without (with) municipality-of-incorporation time trends estimating a model that includes *Fisc.Rule.Exp.* which represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, a dummy *Post* indicating years after 2007 and their interaction term. In column three bias-adjusted estimates β^* are computed with the following formula, and implemented in STATA with the .ADO file PSACALC2: $\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}]\frac{R^2_{Max}-\widetilde{R}^2}{\widetilde{R}^2-\mathring{R}^2}$ with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being the R^2_s from the model with and without city-year FE, respectively. δ is set to 1 under the assumption that firm specific time-varying unobservables are at least as important as the observables to estimate treatment effects. Rows reports estimates for: *Rev.Proc.*, which is the value of procurement won in a year (in 100,000 euros); *Capital*, which are the firm total annual physical assets (in 1,000 euros); *Labor* which are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table D.5: Estimates of the effect of exposure on revenues from procurement, capital and labor for firms incorporated in special status regions

Dep.Var.	Rev.Proc	Capital	Labor	Capital	Labor
Model	FE	FE	FE	FE-IV	FE-IV
	(1)	(2)	(3)	(4)	(5)
Fisc.Rule.Exp.XPost	-0.021	-4.688	1.435		
	(0.042)	(6.110)	(1.840)		
Rev.Proc				220.753	-67.550
				(538.536)	(144.752)
Observations	676	676	676	676	676
Company FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
CityFE*trend	YES	YES	YES	YES	YES
Mean Y	3.486	1122	701.7	1122	701.7
St.Dev.Fisc.Rule	14.07	14.07	14.07	0.632	0.632
Eff.Fisc.Rule.Exp. (%)	-8.571	-5.880	2.876		
First-Stage F-Stat				0.125	0.125

Notes: In columns 1-3, the table reports estimates of the effects of exposure to the fiscal demand shock on firms revenues from procurement, capital accumulation and labor for firms incorporated in special status regions. Columns 4,5 report IV estimates using *Fisc.Rule.ExpXPost* as an instrument for *Rev.Proc.* First-Stage F-Stat is the first stage statistics for the relevance of the instrument. *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp* represents the exposure to the fiscal demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. All estimates include Company, Year and city of incorporation-by-year fixed effects. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coeffcient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule* and Mean Y. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011 and incorporated in special status regions *(Friuli, Trentino, and Val d'Aosta)* .

Table D.6: Bankruptcy

Dep.Var.	Exit	Exit	Exit
Model	OLS	FE	FE-HT
	(1)	(2)	(3)
Fisc.Rule.Exp.XPost	-0.0001	0.010	-0.0001
	(0.007)	(0.008)	(0.009)
Fisc.Rule.Exp.	0.005		
	(0.004)		
Post	2.452***		
	(0.223)		
Observations	22,947	22,891	22,889
Company FE	No	Yes	Yes
Year FE	No	Yes	Yes
CityFEXtrend	No	No	Yes
Mean Y	2.262	2.262	2.262
St.Dev.Fisc.Rule	25.41	25.41	25.41
Eff.Exposure (			
%)	-0.514	10.72	-0.319

Notes: The table reports estimates of the effects of the demand shock on firm Bankruptcy/exit defined as the probability of going bankrupt in a given year. *Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2007. Col. 1 (2) [3] reports OLS (FE) [FE-HT] estimates (with firm and year fixed effects) [municipality-of-incorporation time trends]. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule on Treated (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post**St.Dev.Fisc.Rule and Mean Y. SEs are clustered at firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

Table D.7: Use of inputs not affected by firing costs

Dep.Var.	Rev.Proc	Capital	Labor
Model	FE-HT	FE-HT	FE-HT
	(1)	(2)	(3)
	Panel A: High duration of labor disputes		
Fisc.Rule.Exp.XPostXDur.Lav	-0.001	0.004	-0.001
	(0.001)	(0.003)	(0.001)
Fisc.Rule.Exp.XPost	-0.1082**	-4.1641**	0.1100
	(0.010)	(1.255)	(0.246)
	Panel B: Large firms subject to EPL		
Fisc.Rule.Exp.XPostXEPL	-0.0234	2.7155	-0.6975
	(0.063)	(1.985)	(0.725)
Fisc.Rule.Exp.XPost	-0.1086**	-1.4960	0.5964**
	(0.026)	(0.877)	(0.293)
Company FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The table reports estimates of the effects of exposure to the demand shock on firms revenues from procurement, capital and labor. *Rev.Proc* are the revenues from procurement (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp.XPostXDur.Lav. (Fisc.Rule.Exp.XPostXEPL)* is a triple interaction term between the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, the dummy post, and the average duration of labor disputes in the court of appeal where the firm is incorporated, in 2007 (a dummy variable for with more than 15 workers subject to employment protection laws). In Panel B, we consider the sub-sample of firms with employment between 10 and 20 workers before 2008. All estimates include firm, year and municipality-of-incorporation time trends and the interaction term *PostXDur.Lav (PostXEPL)*. SEs are clustered at firm level. Significance at the 10% (*), at the 5% (*), and at the 1% (**). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

Table D.8: Effects of the demand shock on outsourced services

Model	O.Services OLS	O.Services FE	O.Services FE	O.Services Parellel Trend	O.Services Leads&Lags
Fisc.Rule.Exp.XPost	4.345*	2.970*	2.393**		
	(1.928)	(1.706)	(0.942)		
Fisc.Rule.Exp.	-18.032***				
	(2.099)				
Post	-148.702**				
	(65.913)				
Fisc.Rule*Year				0.395	
				(0.477)	
Fisc.Rule.Exp.*Year2004					-0.648
					(1.926)
Fisc.Rule.Exp.*Year2005					-0.086
					(0.895)
Fisc.Rule.Exp.*Year2006					-0.308
					(0.877)
Fisc.Rule.Exp.*Year2008					1.559***
					(0.477)
Fisc.Rule.Exp.*Year2009					1.902**
					(0.953)
Fisc.Rule.Exp.*Year2010					2.727**
					(1.156)
Fisc.Rule.Exp.*Year2011					2.726**
					(1.115)
Observations	22,947	22,891	16,004	10,060	22,889
Company FE	NO	YES	YES	YES	YES
Year FE	NO	YES	YES	YES	YES
CityXYearFE	NO	NO	YES	YES	YES
Mean Y	1212	1212	1212	1212	1214
St.Dev.Fisc.Rule	25.41	25.41	25.41	25.41	
Eff.Fisc.Rule.Exp. (%)	9.110	6.228	5.017	5.017	
No-pre-trend					0.955

Notes: The table reports estimates of the effects of exposure to the demand shock on the firm total costs for outsourced services (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2008. *Year2004-Year2011* are time dummies interacted with *Fisc.Rule.Exp.* All estimates include firm and year fixed effects. Columns 3-5, include municipality-of-incorporation time trends. In Column 4 *Fisc.Rule.Exp*Year* is the interaction term between *Fisc.Rule.Exp* and a linear trend and it is used to test for linear trends in the pre-2008 sample. *Parallel trend test (p-value)* is the p-value of the joint test for all the leading terms equal zero. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post**St.Dev.Fisc.Rule and Mean Y. Significance at the 10% (*), at the 5% (*), and at the 1% (**). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Online Appendix

Table O.1: Impact of the fiscal shock on infrastructure spending controlling for different functions of municipal population

	(1)	(2)	(3)
Fisc.Rule*Post	-5.946***	-5.522***	-5.651***
	(2.023)	(1.248)	(1.317)
Population	0.003		
	(0.004)		
Population^2	-0.001***		
	(0.0001)		
Log-Population		2.685	1.666
		(2.804)	(2.644)
Observations	48,120	48,120	48,120
Sample	All	All	All
Municipal FE	YES	YES	YES
Year FE	YES	YES	YES
Mean Y_treat-pre	24.19	24.19	24.19
Eff.Fisc.Rule on Treated (%)	-24.58	-22.82	-23.36

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. In Column 1, we control for population in 1,000 inhabitants and its square; In Column 2, for log-population and in Column 3 we also include 12 dummies for binned categories of population size. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

Table O.2: Robustness: different data transformations

	Municipal data			Firm data								
Dep.Var.	Tot.Val.Proc.	Tot.Val.Proc.	Tot.Val.Proc.	Rev.Proc	Rev.Proc	Rev.Proc	Capital	Capital	Capital	Labor	Labor	Labor
Dep.Var.Spec	Levels	Logs	IHS	Levels	Logs	IHS	Levels	Logs	IHS	Levels	Logs	IHS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fisc.Rule.*Post	-5.459***	-0.184***	-0.280***									
	(1.199)	(0.029)	(0.025)									
Fisc.Rule.Exp*Post				-0.105***	-0.288***	-0.206***	-5.177***	-0.051***	-0.041***	-0.069	0.019**	-0.015
				(0.007)	(0.021)	(0.009)	(0.685)	(0.014)	(0.013)	(0.205)	(0.009)	(0.013)
Observations	48,120	26,724	48,120	22,891	7,174	22,891	22,891	18,983	22,891	22,891	18,550	22,891
Company FE	YES	YES	YES	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	YES	YES	YES	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_treat-pre	24.19	2.381	2.638	3.456	1.504	0.871	617.5	4.928	5.487	410.6	5.344	5.720

Notes: The table reports estimates of the effects of exposure to the demand shock on municipal spending for procurement, and firms revenues from procurement, capital accumulation and labor: *Tot.Val.Proc.* is the annual total value of municipal tenders for infrastructures in all Italian municipalities; *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). These variables are expressed in levels, in logs or in IHS depending on the specification. Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, and it is expressed in levels, in logs or in IHS depending on the specification. In each of the rows, *Post* is an indicator for the years after 2007. In Cols.1-3 estimates are for municipal data, while in Cols.4-12 for firms. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table O.3: Robustness: Post > 2008

Dep.Var.	Tot.Val.Proc.	Rev.Proc	Capital	Labor
	(1)	(2)	(3)	(4)
Fisc.RuleXPost	-6.825***			
	(1.170)			
Fisc.Rule.Exp.XPost		-0.088***	-4.504***	0.012
		(0.006)	(0.597)	(0.204)
Observations	48,120	27,678	27,678	27,678
Mean Y_treat-pre	24.01			
Eff.Fisc.Rule on Treated (%)	-28.42			
Mean Y		3.161	585.8	398
St.Dev.Fisc.Rule		23.91	23.91	23.91
Eff.Fisc.Rule.Exp. (%)		-66.78	-18.38	0.07

Notes: The table reports estimates of the effects of exposure to the demand shock on municipal spending for procurement, and firms revenues from procurement, capital accumulation and labor: *Tot.Val.Proc.* is the annual total value of municipal tenders for infrastructures in all Italian municipalities; *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. *Post* is an indicator for the years after 2008. In column 1 estimates are for municipal data, while in columns. 2-4 for firms. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table O.4: Balance tests for the municipal data

Dep.Var.	Tot.Val.Tenders	N.Tenders	Avg.Value Proc.	Roads	N.Bidders	Winning Rebate	Winners local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: Full-Sample				
Fiscal rule (pop.>5k)	24.71	5.074	4.781	33.72	33.48	19.04	48.82
No fiscal rule (pop.≤5k)	2.867	1.144	2.877	31.23	26.44	17.82	66.40
p-value Diff.	0.001	0.001	0.001	0.049	0.001	0.001	0.001
Obs.	6015	6015	3580	3580	1940	2624	1783
			Panel B: 1k-10k				
Fiscal rule (pop.>5k)	7.965	2.273	4.007	34.13	31.32	17.89	56.13
No fiscal rule (pop.≤5k)	3.426	1.322	3.012	31.28	26.88	17.72	63.95
p-value Diff.	0.001	0.001	0.001	0.104	0.005	0.700	0.001
Obs.	3930	3930	2318	2318	1145	1621	1038
			Panel C: 3k-7k				
Fiscal rule (pop.>5k)	7.263	2.104	3.760	32.35	28.67	17.63	56.20
No fiscal rule (pop.≤5k)	4.936	1.674	3.449	31.68	28.84	17.83	62.05
p-value Diff.	0.001	0.001	0.286	0.792	0.938	0.763	0.074
Obs.	1497	1497	982	982	565	730	485
			Panel D: 4k-6k				
Fiscal rule (pop.>5k)	6.179	1.911	3.544	30.32	27.39	17.60	57.78
No fiscal rule (pop.≤5k)	5.311	1.746	3.590	33.32	27.64	17.10	63.13
p-value Diff.	0.278	0.315	0.920	0.399	0.936	0.532	0.248
Obs.	701	701	462	462	282	341	227

Notes: *N.Tenders* is the number of tenders in a municipality in a year; *Avg.Value of procurement* is the average value of tenders in a municipality in a year computed using the engineers' estimates of the value of the works; *Roads* is the fraction of all tenders for road constructions; *N.Bidders* is the number of competitors submitting an offer; *Winning-Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. Source: Statistics for all the public works tendered in 2007 in Italy.

Table O.5: Results are robust to different clustering of standard errors, and controlling for local demand shocks

Dep.Var.	Rev.Proc	Rev.Proc	Capital	Capital	Labor	Labor
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: cluster at the municipality of incorporation level					
Fisc.Rule.Exp.XPost	-0.105***	-0.101***	-5.177***	-3.732***	-0.069	0.237
	(0.007)	(0.009)	(0.655)	(0.716)	(0.229)	(0.189)
Eff.Fisc.Rule.Exp. (%)	-76.87	-74.46	-21.31	-15.36	-0.427	1.465
	Panel B: controlling for regional demand shocks					
Fisc.Rule.Exp.XPost	-0.105***	-0.102***	-5.187***	-3.711***	-0.097	0.226
	(0.007)	(0.008)	(0.700)	(0.822)	(0.213)	(0.173)
Eff.Fisc.Rule.Exp. (%)	-77.50	-74.72	-21.35	-15.27	-0.598	1.399
Observations	22,891	22,889	22,891	22,889	22,891	22,889
Company FE	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
Mean Y	3.456	3.456	617.5	617.5	410.6	410.6
St.Dev.Fisc.Rule	25.41	25.41	25.41	25.41	25.41	25.41
Eff.Fisc.Rule.Exp. (%)	-77.50	-74.72	-21.35	-15.27	-0.598	1.399

Notes: The table reports estimates of the effects of exposure to the legislative change on firms outcomes: *Rev.Proc.* are the revenues from procurement (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2007. Odd (even) columns report estimates with firm and year fixed effects (add municipality-of-incorporation time trends). Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post**St.Dev.Fisc.Rule and Mean Y. In Panel A, SEs are clustered at the municipality level. In Panel B, all the regressions include the fraction of cities exposed to *Patto* in the region of incorporation of each company (and its interaction with the *Post* dummy) and SEs are clustered at the firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.